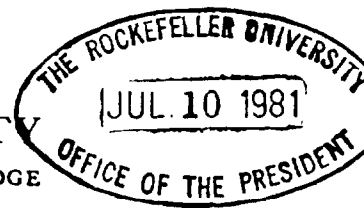


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Edward C. Carter, II Librarian

July 8, 1981

Dr. Joshua Lederberg, President

The Rockefeller University  
New York, NY 10021

Dear Josh:

Whit Bell referred to me your request for some information about the 1946 Cold Spring Harbor Symposium, and shortly after I returned to Stony Brook I received also your letter of inquiry about the same matter. I fear there is very little information to impart to you. I have rechecked the Demerec Papers, and there is no correspondence whatsoever that would throw light on how Demerec arranged the program or whose assistance he had. Vernon Bryson and Albert Kelner were on the staff then and possibly he asked them for advice. He also may have consulted Max Delbrück, who had started the phage course the preceding summer. Perhaps Luria, who had helped Demerec with advice regarding publication of papers on bacterial mutation? Perhaps Rollin Hotchkiss?

It is quite certain, from my own recollection as well as the attached program of participants in the 1946 Symposium, that Avery was present. The McCarty, Taylor, and Avery paper received a very excited reception and caused much discussion, as you may remember. I cannot find my copy, if preserved, of the actual 1946 program. I probably threw it away-- a great mistake, since the Symposium volumes do not reproduce the actual program with the dates of presentation, in proper order, of all papers. Your paper was, I think, presented in the second group, probably on the second day of the symposium, or maybe the third. Would that have been on July 7 or 8? I enclose a xerox copy of the Foreword to the Symposium volume, with attention called to some relevant statements; and also a copy of the first page of the listed participants, where Avery's name is listed. I remember his attendance so well, inasmuch as it was the first time I had ever seen him, or you, in person. I also enclose a reprint of an article of my own (1974) taking Gunther Stent to task because he included Avery as an example of "prematurity" in scientific discovery. I have since then discussed the issue with him, and now believe the difference of opinion is mainly semantic, or one of emphasis. He would rate every discovery that is not immediately acclaimed as "premature," whereas I think that, especially in cases of a revolutionary idea, it takes some time to substantiate a new concept and produce overwhelming evidence of its correctness. I still think the Avery, McLeod, and McCarty paper of 1944 received widespread attention from geneticists and started a great debate over the respective merits of DNA and protein as the genetic material.

With best regards,

*Bentley*  
Bentley Glass